

Is Translation the Problem? Some Reactions to Critchfield (2011)

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The crux of Critchfield's (2011) article is that there have not been enough translational contributions, and as a result societal support for basic research in behavior analysis is waning. Critchfield suggests, therefore, that to remain viable as a research enterprise, more attention needs to be paid to the translational (read "immediate practical") implications of basic research in behavior analysis. I hope that is the case, but there are reasons to doubt that it is.

As noted by Critchfield, most basic research in science in general does not lead to any obvious societal benefit (cf. Wade, 2010). That becomes clearer when one considers sciences other than psychology. Consider as three examples, astronomy, paleontology, and mathematics. The first two are sciences that produce knowledge that most assume will almost never have practical benefit. Mathematics has produced benefits for the sciences, but now modern mathematics is centuries ahead of the other sciences in the techniques being developed, so certainly there is little soon-to-be-felt benefit. Society, nevertheless, continues to support research in those fields. Any account of why basic research in behavior analysis is not being supported has to rest, therefore, at least to a significant extent, on considerations other than the fact that it is perceived as too esoteric and disconnected from practical concerns. Perhaps, however,

because behavior is at the root of many, many of the world's problems, the public may be more demanding of practical outcomes of research aimed at understanding it.

A first point of possible disagreement with Critchfield is the degree to which research in the tradition of the experimental analysis of behavior (EAB) has yielded translational benefit. Critchfield argues not much; I, in contrast, would argue a heck of a lot, a view I base on a longer historical perspective. In fact, I would wager that no other area of psychology has produced as much translational benefit as has EAB. For example, I look at more than 40 years of publications in the *Journal of Applied Behavior Analysis (JABA)* (not to mention a very large number of publications in myriad other outlets) as being essentially *all* translation of EAB. It is useful to remember that *JABA* was created by the Society for the Experimental Analysis of Behavior precisely to help illustrate the translational potential of research in EAB. The resulting body of research published in *JABA* not only has illustrated translation that is of high societal value in clinical and other practical realms, but it also has revealed the broad generality (e.g., the wide array of events that can serve effectively as reinforcement) of concepts based on a limited number of exemplars (mainly food or water presentation) in the basic research literature. Thus, I see no lack of translation of research from basic research laboratories to nonlaboratory environments.

If lack of translation is not a problem, then what is the problem?

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Here, I am in complete agreement with Critchfield that there is a problem, one surrounding support for research in basic EAB, but I do not agree that the problem lies with this research not being directed to a sufficient degree at translation. I think the problem is much deeper than that, and more multifaceted.

The difficulty is tied, at least in part, to the diminishing support for behavioral research with nonhuman animals that is occurring across different approaches to the investigation of basic behavioral processes. Recently, for example, traditional homes for support of research on learning processes in nonhumans, the National Science Foundation and the National Institute of Mental Health, have both sharply and deliberately curtailed financial support of such research, much of which would not be characterized as part of EAB. Hence, it is not a problem particular to EAB. EAB is just one of the players.

A second part of the problem lies with those who are involved in the awarding of basic research support for behavioral science. That group is dominated by traditional psychologists who, for a variety of reasons, give little respect to behavior-analytic approaches. Those areas in which behavior analysts have had success (grudgingly given, I would argue) historically are those in which those in control of the resources have had to admit that the behavior-analytic approach had been effective in understanding whatever phenomenon was under investigation. Thus, anti-behaviorist sentiment is still present and cannot be ignored.

A third possible origin of the current situation is what is akin to a drive for instant gratification. Seeing this as a problem is based on taking a longer historical view, one that sees psychology and EAB as sciences still in very early stages. Psychology, for example, still has no agreed-upon basic subject matter, basic laws, or

basic measures. (One of the attractions of behavior analysis, in fact, is that as a subdivision within psychology it has some agreement about what those entities are or should be.) Be that as it may, however, the drive for relatively immediate practical results is what motivates much current thinking about societal support for basic behavioral science (physics seems to be doing okay, however, without such worries). A problem is that it is not very likely that doing research that is claimed to lay the foundation for solutions to particular societal issues will actually result in such solutions. That is because the science is still too young. And in the long run, promising what cannot be delivered might well be more damaging than being honest about the current state of knowledge and what is likely to be achieved by any particular research project or program. Behavioral issues in the everyday world are extraordinarily complex, so promising translational relevance puts one at risk (probably high risk) of making promises that will be broken. That, interestingly, is never a problem for pure basic research.

It is likely too soon to be making promises about how research on behavior will lead to amelioration of behavioral problems, because one of the most fundamental issues in human behavior, how the role played by a verbal repertoire develops, is still largely a complete mystery. There is little wonder why that is, either. First, research on the topic appears to be impossible (at least as the processes are currently understood, which unfortunately is at the level of face validity) to conduct with nonhumans. Second, it is a very, very difficult technical problem because it is impossible, both practically and ethically, to do the kinds of definitive experiments with humans that could begin to unravel what kinds of experiences result in the various kinds of relationships that can be observed.

To list but a few questions, how and why does what Skinner (1969) called *rule governance* develop? Why are some people easily duped by verbal stimuli and others not? Why do so many reason illogically? Why don't people always do what they say they are going to do? What are the roles of so-called self-rules in governing behavior? The number of important questions is very large. Because we cannot do true experiments on the development of these processes, we can only observe them as they happen and examine their characteristics after it has. Cognitive psychologists have always taken this established repertoire as their starting point in examining such things. Are behavior analysts obliged to do the same thing, or does an account that focuses on the experiences that presumably result in the final repertoires offer an avenue to alternative approaches? Some research on relational frame theory (e.g., Murphy, Barnes-Holmes, & Barnes-Holmes, 2005) and other approaches to verbal control may be of that sort.

In some respects, I think Critchfield is arguing more for research with humans than he is for research that is translational. If that is so, then the discussion would more profitably be aimed not at getting more EAB researchers to be directed by translational issues (with the inherent problem of deciding what is of practical import), but instead in devising ways to circumvent the seemingly intractable problems associated with studying effects of experience in subjects who have long histories of uncontrolled experiences. The ongoing disintegration of psychology as a field (read almost any introductory psychology textbook for evidence) suggests that mimicking the methods most widely used so far (group-average comparisons and statistical significance testing) is unlikely to be profitable. It can be argued that a key contributor to the disintegration of traditional psychology has been the

employment of significance testing of group means, which as usually employed leads to aimless science (as noted by Meehl, 1967, 1978). Perhaps, therefore, it is here that EAB research on adult, verbally capable humans can help to rescue psychology by illustrating how intersubject comparisons can be accomplished without abandoning the focus on behavior, that is, what individuals do. That activity, by the way, would be constituted mainly of pure basic research, but on humans. As an example of one kind of problem, Critchfield suggests that a behavior analyst might be loath to study self-editing using procedures developed by psycholinguists because those procedures do not produce in every subject the phenomena from which self-editing is inferred. (Actually, I find it hard to believe such avoidance would occur.) Variability, either within or between subjects, is a result of uncontrolled variables, so such a circumstance presents an opportunity to attempt to identify the sources of the differences among subjects, and maybe even eliminate them. Certainly, a behavior analyst would be loath to do one thing, and that is average the data of the two categories of subjects.

Overall, my view of Critchfield's call for more emphasis on translational research is that it is really to urge more behavior-analytic research on verbally competent humans. I find that an admirable (if daunting) call. Behavioral research with nonhumans is not in current vogue across all approaches to the study of behavior, not just behavior-analytic ones, despite its obvious advantages with respect to control of the primary domain of independent variables—experience. Consequently, behavior analysis would do well to bring its viewpoint, and more important, its methods, which focus on individuals, to research on normally developing or developed humans. The eventual success of that endeavor, however, rests

on advances in experimental techniques that overcome the problems associated with experimental subjects who have extensive, relevant histories.

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